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# INFECTION-DISEASES

IN THE ARMY

CHIEFLY

WOUND FEVER, TYPHOID, DYSENTERY AND DIPHTHERIA

BY

PROFESSOR RUDOLPH VIRCHOW.

TRANSLATED FROM THE GERMAN BY

JOHN JAMES, M.B. LOND., F.R.C.S.

LONDON

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## P R E F A C E.

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THE discourse of the illustrious Professor Virchow, which I have endeavoured to translate, was given at the anniversary of the Pepiniere. It treats of subjects which are now engaging public attention as well in civil as in military life. And since the smaller essays of great teachers are often among their most interesting works, I venture to hope the pamphlet here presented in an English dress may not be unacceptable to many readers.

THURLOE SQUARE,  
March, 1879.



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## INFECTION-DISEASES IN THE ARMY.

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AN anniversary like the present, wherein a great State-Institution revives the history of its foundation, deserves not merely to be remembered. If the critical eye of an impartial judge scans with satisfaction and even with thankfulness, the long space of eight decades, during which this Institution has furnished to the army with constantly approximating completeness, an indispensable personal contingent; so must it not dare to shrink from further examining whether the training which this Institution supplies to its pupils, attains such breadth and solidity, as well scientifically as practically, that the army may confidently reckon, amidst all the changes of war and peace, to receive in these gentlemen reliable helpers and advisers. Not without a deeper mean-

ing, was the name Pepiniere assigned to this establishment, at the time when it was founded. It was constituted for a seedling school wherein the students should be educated to something higher than practical routine. It was designed to elevate the then degraded surgery, by accomplishing an union between the dissevered branches of medical instruction: it was designed to yield to the army not mere field-cutters, but physicians, who in their previous training, had already imbibed the elements of a true humanity, and who in their technical development had acquired a fitness not only to carry out the lessons received, but also to take their part in the onward movement of science with intelligence and responsibility. It was a momentous problem. It has been solved. But whilst it has been thus solved, relative to this institute, another and a more general goal was reached. That separation inherited from the middle ages between surgeons and physicians, which in a certain sense still

obtains in England, became by degrees generally abrogated, and the unity of universal medicine—this stamp of modern times,—has not been won without the determining influence of Prussian medico-military organization.

The modern medicine—and even that is a noteworthy circumstance—is scarcely as old as this Institution. The great authorities of past centuries are to-day almost forgotten. Frederick Hoffmann and Ernest Stahl, Van Swieten, Sauvages, and Cullen—they arouse now-a-day only the interest of historians. Under the mighty surging of the French Revolution, after the other natural sciences, and particularly chemistry, had already made considerable progress on the sound bases of observation and induction, began also the pioneer labours for the reconstruction of medicine. Exactly at this epoch occurred the founding of the *Pepiniere*. The foundation was an omen of the coming time. Since this epoch one medical school after another has sprung up, each with

improved methods and better aids to knowledge ; from decade to decade the sum of facts, the number of possibilities in science have become vastly multiplied. A well-ordered empirical knowledge has taken the place of philosophical speculation. Diagnosis has gained the firm foundation of pathological anatomy ; prognosis the assurance derived from statistics, and in the treatment as well of surgical as of internal lesions, dogmatical assertions founded mostly on arbitrary assumptions and generalising interpretations, give place as guides for treatment to the teachings of experimental, and even local curative indications. Nowhere is the contrast between the past and the present more marked, than precisely in the matter of therapeutics. Whilst surgical treatment is ever widening its limits, the territory appropriated hitherto to the so-called internal medicine, narrows in a manner that a short time ago would appear incredible ; whilst the several operations, both in boldness and comprehensiveness, range

far beyond the recognised lines; also, even in external surgery, which chiefly concerns the wounded soldier, has a preservative tendency so established itself, that it now seems a most extreme measure to effect the removal of a limb altogether. And likewise in internal medicine. At the time of the war of liberation, blood-letting was looked upon as not only an admissible, but even as an useful remedy in typhus, and, therefore, one to be generally practised. At this moment we hold it a crime to employ general blood-letting, where the loss can be anywise avoided; not only typhus but inflammation of the lungs is cured without venesection. In truth, we are now engaging ourselves earnestly, how to transform the great operations, which, in a certain sense were denominated "bloody," into bloodless operations.

It is owing to the same preserving, but in no way conservative tendency, that thoughtful men now direct their attention more and more to a hitherto almost totally neglected side of treat-

ment, viz., the preventive or prophylactic. It is the same as that which comes before us with the pompous title of Public Health Service. Here the physician walks abreast of the functionaries of government, and even shares certain problems with the military commander.

The care for pure air, and pure drinking water, for healthy food, and healthy camping grounds, is obligatory, not merely on the medical administration. In many instances the action of the quarter-master is still more decisive, and even the general, who knows there are evils not unavoidable, which may decimate an army, when they once take a firm hold, will often have to deliberate in war and peace, whether his orders coincide with the laws of health or not.

Such vast transformations—and many more examples might be cited—are essentially brought about by the progress of science. One can likewise assert that it has been sharp necessity, this keenest of monitors, which has opened men's eyes

through the heaviest visitations, so that they are compelled to notice what, to speak accurately, they would not see. Yes, it is astonishing, what schools of suffering the armies have had to pass through before the truth became commonly acknowledged! Thus, in the Crimean war, the French army lost one man out of every three, in their whole army, and it is calculated that of the 96,615 men who forfeited their lives, only 10,240 fell before the enemy; about an equal number of wounded died in the Hospitals. The rest, more than 75,000 men, fell a sacrifice to disease. In the American civil war, 97,000 men died in battle, and 184,000 from epidemics and sicknesses. What a huge mass of pain and suffering, what a sea of blood and tears stands revealed in these figures. But also what a heap of fallacious regulations, of prejudices and misunderstandings. It is unnecessary to lay bare here the long list of these sins and mistakes; fortunately it is sufficiently well known in order to serve as a

warning for others. But it must also be said that it was not necessity alone which exposed the evil and brought redress. That the French learned little or nothing in the Crimea, and the North Americans so much in their civil war, that from that date onwards begins a new era of military medicine—this depends not on the magnitude of the necessity which the Americans had to undergo, which in truth was not greater than the French underwent in the Crimea. It was far more the critical, genuinely scientific spirit, the open mind, the sound and practical intelligence, which in America penetrated step by step every department of army administration, and which under the wonderful co-operation of a whole nation reached the highest development that, relative to humane achievements, had hitherto been attained in a great war. Whoever takes up and looks into the comprehensive reports of the military medical staff, will be again and again astonished at the richness of the experiences chronicled



therein. The utmost accuracy of detail, painstaking statistics embracing the minutest particulars, an erudite exposition comprehending every aspect of the practice of medicine, are here united in order to preserve and transmit to contemporaries, and to posterity, in the most thorough way possible, the wisdom purchased at so tremendous a price.

In the late French war, the German armies, out of 913,967 men, sustained a loss of 44,890; whereof 17,572 died in battle, later, 10,710 succumbed to their wounds, and 12,253 perished from epidemics and sicknesses,—certainly a very favourable contrast. But we had before us the experiences of two recent wars, which scientifically and administratively had been well discussed and utilized; we had the invaluable reports of the Americans, and lastly, we had the German science.

In truth it must be acknowledged that German science during the half century of peace, whereof

the later years underwent some passing interruptions in insignificant campaigns, and several mobilisations, had almost lost its connection with the problems of war. Even surgery, which from its very nature has at all times chiefly engaged itself with wounds and injuries, had to so great an extent taken refuge in the hands of specialists, that when the call to arms sounded, it appeared necessary to summon the Professors of particular departments, as working reinforcements in military surgery. It seemed yet worse with regard to the subject of internal medicine. Scurvy had so absolutely vanished out of the hospitals, that we were wont to reckon it among the obsolete diseases ; and first the Crimean war, and that of the Baltic, demonstrated that like causes ever produce like results. The infecting war Typhus, which, since the campaign of 1812, had been held in the worst repute, at that time spoken of as the Russian fever, had become so rare on the Continent, that one gradually confounded it with

abdominal typhus ; the famine pestilence in Upper Silesia first resuscitated it as a real object for medical investigation in Germany.

Only in one direction, was attention kept earnestly on the alert—which almost equally concerns every branch of medical practice ; I refer to the proper hospital or noso-comial diseases. The destructive wound fever of surgeons, the suppurating fever of physicians, the puerperal fever of gynecologists, all lead simultaneously to the mysterious region, wherein science and practice stand equally impotent in the face of a dark and over-powering force.

The same disease that in the Lazarettos carried off the wounded in war, raged at one time more, at another time less, from year to year, in the sick and lying-in hospitals ; only the definition of its nature, and accordingly the name assigned to it was changed. At the time of the war of liberation it was reckoned among the adynamic fevers. As, however, through Broussais and Schönlein, the doc-

trine of essential fevers became defunct, when pathological anatomy began to invade even this field of enquiry, there arose the name of phlebitis, because one believed oneself to have detected in the veins the proper seat and starting point of the evil. Even after the ambiguous name of pyæmia was superadded, the original vein affection still so far remained as the leading idea, that the great French investigator, Cruveilhier, gave utterance to the maxim: "*La phlébite domine toute la pathologie.*"

It was about this time that I was appointed by the then leader of this Institution, the present much esteemed chief of the German military medical service, to undertake the scientific enquiries at the Charity; and at the present moment I venture to recall, that it was granted me, as on August 2nd, 1845, this Institution celebrated the feast of its first half century, to expound from this spot, in a festal oration, the first communication of my investigations in phlebitis and pyæmia.<sup>1</sup>

On that occasion I was already enabled to demonstrate, that neither the name of phlebitis nor that of pyæmia were appropriate, that the question turned far more on the one hand as to clots in the blood-vessels and their subsequent history, and on the other hand, as to an infection of the blood, through impure or even putrid material, which infection I designated later as thrombosis and embolism, ichorraemia and septicæmia. But I was not able to state accurately what injury it was which this ichorraemia and septicæmia ushered in.

Thereupon followed immediately another discussion—A large series of the worst types of disease, as puerperal fever, dysentery, even hospital gangrene build on the assailed localities—pseudomembranes, from which the further disintegration of the tissues proceeds. Rokitansky has denominated the greater number of them as croupous. However, a more minute microscopic research elicits a very characteristic distinction.

Whilst the croup membranes manifest themselves as an elimination of fibre-material which, as a layer capable of being stripped off lies next to the affected surface, but leaves this unattacked; in another series of cases, there is manifested a persistent depositing of the finest nuclei in the tissue itself, and if the deposit loosens as a "skin," it leaves behind an ulcer which, through constantly renewing deposit, extends itself only too easily at increasing depth. I named these groups of diseases by a title, first chosen by Bretonneau for a single locality, diphtheritic.<sup>2</sup>

The great cholera epidemic of 1848, soon gave opportunity for shewing diphtheria in the intestines, vagina, and other mucous membranes.<sup>3</sup> I found it in small-pox and scarlet fever, and even in the most manifold infectious processes; its connection with bad types of erysipelas, with deep-seated gangrenous affections, and the more serious cases of internal inflammations became disclosed,<sup>4</sup> and so I came at length by another

route, to a form of infection which, yet more than that of thrombosis, has an ichorous and putrid character.

Under such conditions, the Ancients already imagined an impurity of the fluids. They called the tainting substance miasma (from *μῑαννω* inficio). I have consequently termed the whole class infection-diseases. Others have preferred another likewise ancient epithet, namely, the comparison with fermentation, *ζυμωσις*, derived from the leavening and fermenting of bread. They use the name zymotic diseases. A certain number of these diseases are also contagious; they plant themselves from man to man, like a brand that kindles from house to house through infection. But what then is this miasm, or if you like, this contagium? For hundreds of years the notion, every now and then, has lighted up that it is an independent being with its own vitality and fitness for propagating. The doctrine of contagion, *vivum s animatum*, so often rejected,

emerges with ever-augmented strength, as often as the scientific exploration of nature revives in the domain of pathology. Although the tendency of enquirers to premature generalizing of an experience, valid to a limited extent, has here also too frequently led them astray. The discovery of the itch insect, the demonstration of the parasitic nature of disease of silkworms, have proved even in the nineteenth century, how easily the judgment of otherwise careful observers, may be misled, and how rashly they incline to attribute an unjustifiable breadth of significance to a newly-acquired fact. But, in the long run, who will blame them if their conjectures become ratified by ulterior observation? Such a confirmation has been given by the history of carbuncle, a mainly epizootic disease, that in the first instance attacks the herbaceous mammifera, and thence easily transplants itself upon the human body. The recent sickness among the fallow deer in the Grunewald has brought this danger beneath the



eyes of all. In the year 1855, Pollender published his first report, on the occurrence of microscopic rods in the blood of cattle affected with carbuncle. Brauell and Davaine very soon extended the acquaintance with these structures, and nobody now doubts that the point turns on the existence of minute vegetable growths of the genus bacterium. They are found in the blood, in the spleen, in the buffy coat, and in external carbuncles. Their transfer in the way of inoculation, signifies as much as transfer of the disease. The carbuncle miasma, the carbuncle contagium, so we conclude, is also bacterium, a very low vegetable form of immeasurable multiplying power.

A not less astonishing observation was first made in the previous year, in an epidemic sickness, the so-called recurrent fever, by my coadjutor Dr. Obermeier, alas, so early a victim to his scientific zeal. In this remarkable disease, the ordinary precursor and companion of contagious

typhus, he managed to find in the blood during an exacerbation, a minute plantkin, moving and swinging in very rapid vibration, the spirochaete which vanishes again along with the disease. The carbuncle, as well as the recurrent fever, have signalised this: that the plantkins occur in the blood, and that their peculiar shape and their relative magnitude, very much facilitate their detection. But the microscope shews us in numerous other infection-diseases, not to say in all, with more or less constancy, smaller structures, unequal *inter se*, and with less distinct characters, variously defined, at one time round, at another time elongated, isolated, congregated in rows, or in large groups, partly in excreted fluids, partly in tissues of the body, moving, or at rest. Ten years ago, one was accustomed to tack them to the genus vibrio, then assumed to be animal; after the report of Hallier the name changed to *micrococcus*, and the opinion of their vegetable nature became current; at length Ferdinand

Cohn placed them under the name of *ball bacteria*, by the side of the *rod bacteria*. Many of them aptly called *point bacteria*, are so small, that they stand on the very limits of visibility, even under the strongest magnifying power. Nor have we yet succeeded in determining for these infinitesimal structures, distinguishing characters, by which it might be possible to recognise them on all occasions, as living elements, as the finest organisms. Their distinction from mere nucleoli, from ultimate particles of dead organic substance, is therefore oftentimes impossible, and the good faith, as well of the observer, as of the experimenter, supplements what is lacking in inductive evidence. Many a day will yet probably pass, before a reliable scientific conclusion will be established for all cases, and a general concurrence, as to the limits of microscopic territory, will be attained. Nevertheless, even now, if one excludes the doubtful cases, a large number of most important facts remain; and it is precisely Mili-

tary Medicine, which is bound to share largely in the efforts to estimate their value and significance.

In the first line stand the diphtheritic processes, and in correlation with them the erysipela-tous, especially the erysipelas malignum. That nuclear deposit in the diphtheritically attacked tissues, of which I previously spoke, has manifested itself more and more as parasitic. What we used to describe as simple infiltration or exudation, shews itself as a dense heap of micro-organisms, whether you call them vibrios, micrococcus, or ball bacteria. They penetrate into the tissues, and even into the cells, and kill them. Already, twenty years ago, the celebrated Italian anatomist, Pacini, spoke of milliards of vibrios, which fill the intestines of cholera sufferers, and relative to which, he assumed that they pastured on the villi. Keber found them in the lymph of small-pox. Basch, the physician of the unfortunate Emperor Maximilian, recognised them

in the Mexican dysenterics. Every year has elicited new examples, and we can now well declare that, in the infection-diseases of man and beast, a vast unexplored region of independent life has been disclosed.

This region constitutes a province of medical botany. Since taken altogether we may justly regard these infecting organisms as standing in nearer relationship to the forms of plants than to any form of animal, notwithstanding they now and then exhibit appearances of moving. We are at present a long way past the time, when independent motion was considered a distinguishing mark of animal nature, and when all very small beings, with such motion, were called infusoria. Self-moving, is very far from being necessarily voluntary motion, and this alone is an infallible sign of an animal. Häckel's attempt to establish between animals and plants, an intermediate kingdom, that of the *protists*, may be somewhat justified for other beings; the parasites

that concern us, stand nearest to the forms of mould which belong to the class of fungi, and many of the most characteristic kinds, manifest such exterior aspect, properties, and build, as are acknowledged to appertain to mould. I indicate in this relationship the coloured kinds of mould, whereof several exhibit physical characters so striking, that in former times they were regarded as real wonders. As to the *blood mould* which grows not unfrequently quite suddenly and massively in the shape of drops and flakes, on bread and other substances largely compounded of meal, Ehrenberg has already shewn it to be the basis of the ancient miracle, "the appearing of blood on the host." The species, *mona prodigiosa*, delineated by this old microscopic authority, belongs to the same genus of ball bacteria, as that which now occupies us. It is a very small and elementary plant that grows with incredible rapidity, and thereby secretes a peculiar vegetable pigment which, externally, truly resembles colouring

matter of the blood, yet in fact is totally different. Other moulds appearing under pathological conditions on the human body, namely, such as develop on suppurating wounds, produce a blue, and others a blackish, and others again an orange-coloured material: for anyone accustomed to examine the variously tinted fungi on the leaves and stems of trees, shrubs, and other growths, a by no means unusual phenomenon.

Unfortunately this department of botany is as yet little cultivated. The uncommon smallness of the objects, the difficulty of isolating them from one another, the frequent mixing of several or indeed many kinds on the same spot, oppose extraordinary hindrances to a trustworthy study. The "pure cultivation," initiated with most scrupulous care, and undertaken with the design of following out the complete history of a single fungus, from its first germ to its latest development, and fructification, ever and constantly breaks down, under the invasion of newer kinds

of fungi, whose elements one had not before seen in the culture beds. Indeed the thought that, out of the organic substance in which the culture takes place, the new elements are first constructed by an act of creation, or as it is somewhat illoyally termed by *generatio equivoca*, constantly finds new disciples. In fact the speculative theory of an Epigenesis, thus vouched for, or as one more recently states it of an Abiogenesis, has nowhere so much probability in its favour as exactly relative to these in some sort hardly visible creatures.

Sober observation, based on experiment, repels all these hopes. Every test of the Epigenists is checkmated by a still better test of the Pan-spermatists. The *a priori* possibility, as far as it seems philosophical thus to speak, receives no empirical confirmation. Even the minutest organism makes its first start, in the way of regular heirship, from a predecessor, and if this organism, in the sense of pathologists, is a miasm, or a



contagion, it results that no such miasm or contagion springs *de novo*. It must of necessity have already existed somewhere in the world, and if we can detect and destroy it here, so must the disease dependent thereon come to an end.

Strangely enough the history of epidemics, manifests a relationship apparently antagonistic to this proposition. No portion of history is more fitted to show this than is the history of war. Only too frequently is it campaigns, sieges, assemblages of soldiers of the most different kinds, which constitute foci of great and occasionally unheard of epidemics. From the siege of Troy, and the Persian wars, from the campaigns of Alexander, and Marcus Aurelius, to the sieges of Sebastopol, Gaeta, and Metz, to the North American and Bohemian campaigns, how many times have formidable diseases broken out among the armies, and through the marching masses by means of sick, and camp followers, spread far and wide among the rest of the population.

When one carefully regards the infection diseases under which martial hosts have to suffer, they may be naturally divided into two great groups, which I might class as those of home growth and those of foreign origin. In the latter series are, the plague, small-pox, and cholera. Nothing supports the assumption that any one of these diseases ever originated on European ground. One may dispute over the time of their first inroad into Europe, or stake an opinion on one or other of the ambiguous sentences of an ancient author. But one durst not allow oneself to be so far seduced by the fanaticism of historical enquiry, as to believe that there have always been epidemics of this kind. The first invasion of the oriental, or let us rather say of the Egyptian plague, in Europe, falls in the year 543; probably soon afterwards appeared the small-pox, whose first outbreak in Arabia, is stated to be about the middle of the sixth century: the first European cholera epidemic burst forth in the year 1830,

after it had begun in 1817 its desolating progress from its Indian home. All these diseases are infectious, though in very different ways. It would be a controversy about the Emperor's beard, if as formerly about the plague, so now about the cholera, one so emphasises the difference of its contagiousness, from that of other diseases reputed contagious, that the very contagiousness itself comes in question. Let there be placed between the first patient and the second, a connecting link, one cannot by disputation set aside the fact that the disease germ emerges from the first individual and reaches the second. The plague is now almost a disease of the past. Although a short time ago the news of fresh outbreaks of this sickness on the Euphrates, and in the Lybian desert, disturbed our minds, we have the consolation of knowing that, with the introduction of planned social regulations in their own native land, the danger-threatening epidemics have come to a standstill.

On the other hand the Bohemian campaign of 1866 in the first instance demonstrated the perils of cholera, the French campaign of 1870-71, the perils of small-pox; in the former case for the army itself, in the latter for the prisoners of war and the resident population.

The other group is that of the indigenous infection-diseases. I shall leave uninvestigated the question, whether the infectious typhus, the so-called spot-fever, or the proper war-typhus, with its attendant, the recurrent fever, belongs to this category, relative to which opinion is liable to divergence within certain limits. According to official returns, there are in the main three diseases which require consideration; *diphtheria*, *enteric fever*, and *dysentery*. In the year 1868, in the time of peace, the army reckoned 2358 cases of diphtheria, 3006 of enteric fever (exclusive of 4850 cases of gastric fever) 327 of dysentery. In the year 1869, there were 1769 cases of diphtheria, 2234 of enteric fever (besides 2266

of gastric fever) and 65 of dysentery. Abdominal typhus therefore is the only one of great importance by reason of its signal influence on the mortality. The army lost in 1868, 329, in 1869, 338 men; these diseases thus reached a mortality of 17·5 and 15·1 per cent. Indeed in the first army corps it amounted to 20 per cent.

In war the relations of things naturally alter. Thus the statistics of the late French war, shew that of the 12,253 fatal cases, more than one half succumbed to abdominal typhus, viz., 6965; 2000 men died of dysentery,—about one sixth. Hence all other death sicknesses comprise only a third of the whole loss. Add thereto the greater part of the 10,710 men, who died in consequence of wounds, as it may well be assumed that the majority of them sank through bad wound-diseases. Thus we obtain a total of nearly 20,000 men, who fell victims to the one or other kind of infection.

The question is, therefore, well worthy of en-

quiry,—whence was this infection specially derived?

Now *small-pox* and *cholera*, as also *abdominal typhus* and *dysentery*, *diphtheria* and *wound-diseases*, are all suspected of being propagated by means of those minute plant-forms which I before referred to. There are found in them all, small *bacteria* and *micrococcus* in vast number; and in fact as to the majority of these diseases these parasites occur in the alimentary tract; viz., in *cholera*, *abdominal typhus*, *dysentery*, in the intestines; in *diphtheria* in the throat. Dysentery may be regarded as an essentially local disease-process of the intestinal mucous membrane; with cholera the general symptoms come to the foreground; but most especially in abdominal typhus. Are then these microorganisms really the proper foundations, or, has as been rather hastily alleged, the essence of these diseases?

The very organisms met with constitute the first important objection to this conception.

Hitherto no one has succeeded in finding such marked distinction between the parasites of cholera, and those of the bacteria of small-pox, and of diphtheria, that one could by each of these diseases recognize special plant-forms with characteristic signs, and in technical language thus fix a diagnosis. The same forms of micrococcus and bacteria which the cholera stool shews, I have seen in motions of patients with spotted fever—even in simple chronic diarrhoea in times free from cholera.<sup>5</sup> Still more was I surprised through another occurrence of the same plantkin. It is a difficult problem in cholera times to diagnose an arsenic-poisoning, because the symptoms in both cases may be very similar. The disease masks as it were the crime. I hoped to detect a difference in the evacuations. But to my great astonishment, I found likewise in the intestines of arsenic victims, the same microrganisms that were alleged to characterise cholera.<sup>6</sup> Just as little is there a possibility, as far as I know, of

distinguishing by means of the occurring ball-bacteria, a variolous throat-diphtheria, from a simple case of diphtheria, or even from one of scarlatina.

In truth, the diphtheritic local processes, constitute nevertheless a striking link between the several infection-diseases. Thus I have shewn in the great epidemic of small-pox of 1858 that, the so-called dells of the variolous pustules are thereby caused, that at this spot a diphtheritic infiltration of the skin takes place; like phenomena occur in the mucous membrane of the air-passages, and the alimentary canal of variolous patients.<sup>7</sup> Thus we find with abdominal typhus occasionally, not merely bowel-diphtheria, but also diphtheritic phenomena of the urinary passages and kidneys. In puerperal fever sometimes, the peritoneal and pleural inflammations which develop in the course of the disease, have a diphtheritic character. Could it not be that the diphtheria in all these cases represents only a complication?



So we all conceived the relationship to be in former times. The diphtheria seemed to us, as the highest grade of a local inflammation, of a bad kind, resulting from the violence of the local process, and likewise from a bad constitution of the assailed tissue or individual.

The older writers had previously grouped these types under the title of gangrenous inflammations. Since, however, it has become demonstrated, that the diphtheritic infiltration is parasitic, the conception as to the essential nature of the local process, has undergone a change, although not the conception of its effect, which is necessarily admitted to be killing, and destroying (necrotising, gangrenising). In spite of this is it not possible that this parasitic affection is a mere complication? We are acquainted with the analogous relations of thrush, an affection of the mouth and throat, and œsophagæal mucous membrane, caused by a distinctly marked thread-fungus, the *ordium albicans*,—while it is known among young

children under the name *schwammchen* or *aphthæ*, as a clearly defined and independent disease ; it is also found as a frequent complication among consumptives, and what is very notable among typhoid patients. In like manner, it is allowable to contemplate diphtheria, as a supplementary incident, which further augments the sum of the already existing disturbance in the infection diseases. Indeed such a conclusion seems quite legitimate when one thinks that, not every case of cholera, abdominal typhus, or dysentery shews real diphtheria.

A similar conclusion may be reached through another path. Even the putrid decomposition external to the body, is, just like the fermentation, attended by the development of little vegetations. Concurrently with this fact, E. Mitcherlich, at a time when it was the custom to class the vibrios, monads, and bacteria, as animals, and the ferment fungi alone as plants, enunciated the distinction between the two processes, viz., that

fermentation is caused by fungi, and that putrefaction is caused by infusoria. Was it not therefore allowable to assume, as did the ancient physicians, in their assumption of a *status putridus*, and a *febris putridus* of the so much dreaded foul fevers, that all these infection-diseases contained a common putrid element? And that in the secretions as well as in the tissues of the body, a certain chemical constitution obtained, favourable to disintegration and decomposition, and the rapid spread of which entailed the multiplication of the putrefactive organisms, lodging in parts so tainted?

Among the secretions there is not one which possesses so marked a tendency to putrify as does the urine. Without the body the mixing of putrid material with the urine suffices to bring about very quickly an alkaline decomposition; an innumerable multitude of minutest plant organisms start forth with astonishing rapidity. This alkaline or urinary fermentation is, as is well

known, one of the most usual complications of bladder-disease, of both men and women, and under such conditions, every case manifests a tendency to diphtheritis of the bladder, and even of the kidneys. Nothing is more common and more troublesome than diphtheritis of the vagina, among such women as suffer from vesico-vaginal fistula.

Next to the urine the excretions of the bowels require to be noted. Both inside and outside the body the fecal materials tend to putrid decomposition and to the copious production of foul organisms. No portion of the body, however, is so much predisposed to diphtheritic affections, as that section of the intestinal canal which contains the matter already passed into a fecal state and destined for elimination, namely, the rectum. Here is the special seat of dysentery; here by preference, locate themselves the diphtheritic manifestations of cholera; here not unfrequently occurs the diphtheria of lying-in women, and of

the victims of the so-called pyemia. Further, here is also the place of selection, the *locus minoris resistantiæ* of the consumptive, and what is certainly very remarkable, of the uremic.

All these forms appear anatomically so similar that, they are ordinarily grouped under the common title of dysenteric processes, or in brief of dysentery,—a pretention which I have always opposed because this notion of dysentery is clinical, ætiological, but not anatomical, and because true dysentery, besides the diphtheritic form which I first demonstrated as such, comes on in a catarrhal follicular form which is *sui generis*. For this reason I have always more insisted on the necessity of recognizing also for true dysentery, the important bearing of retention and putrefaction of fecal-stuff on its development and local rearing<sup>s</sup>. It would lead me too far if I were to attempt a delineation of all the details in the anatomical course of the process at the present moment ; it may suffice to indicate

that in their recognition, is simultaneously given the theoretical declaration, in favour of that ancient, empirical, and on the first aspect, contradictory method of treatment in dysentery, viz., the administration of purgatives. I must not omit to mention further that, in pathological stricture of the rectum, whatever the kind may be, over the narrowed part where a long persistent packing of fecal material often takes place, a diphtheritic affection very promptly sets in, which in violence of local career is comparable only to the puerperal, the sequel being a total necrosis and complete perforation of the wall of the gut. What else can be cited here as the starting cause than the putrefactive decomposition of the stagnating fecal mass?

Such an array of reflections leads fairly to the assertion of the unity of the nature of all those processes which find in diphtheria their anatomical or local expression. One would infer that it only remained to demonstrate the unity of the

nature of the putrefactive organisms. This demonstration Billroth has lately essayed, in a most remarkable botanical investigation, bearing on the promotion of the recently existing impulse towards scientific research in surgery, wherein he comprehensively derives the so-called vibrios, monads, micrococcus, bacteria, and other alleged types, from a single mother-plant which he calls *coccobacteria septica*. Since this plant abounds everywhere in nature, since it is always present in the human body itself, notably in the intestinal canal even with the healthy, so that a primary importation, an infection, or a transference seems hardly required; it is already at hand and wants only favourable conditions for propagating and further growing.

Without wishing to arrogate to myself at this moment a judgment on the fairness of the botanical section of this significant enquiry, I must, however, state that it coincides only in part with my own way of thinking. I hold that it is incon-

trovertible that the ordinary putrefaction organisms suffice to manifest a great part of the local, and a certain part of the general infection-diseases. It is this which constitutes the territory, which we have always conceded, of putrid infection, whereof septicæmia is the highest development. In conjunction with the group occurs at least a large portion of the diphtheritic processes, whose relationship with the putrefactive we long ago asserted, in a certainly nearer connection than hitherto has been commonly assumed, even probably in a much nearer connection than Billroth himself is inclined to allow.

With regard to war medicine, the wound-fever, dysentery, abdominal typhus, and perhaps also throat diphtheria, are the topics that chiefly concern us, being those formerly designated as the *home infection-diseases*. Their outbreak is specially associated with certain localities, and they speedily attain a truly epidemic character. Certain countries and districts, and likewise cer-



tain fortresses, in the history of war, have long been held in bad repute.

When the German kings, on their Roman campaigns, had crossed the Alps and descended with their armies into the Lombard plains, they encountered almost in every instance, simultaneously with the open enemy, a secret and therefore treacherous antagonist—some form of “fever.” More than one German army has succumbed to this insidious foe, after having victoriously overthrown their avowed opponents. Before no fortress, however, have the German armies undergone sadder experiences than before Metz. 1552 and 1870 are the landmarks of a long recital of war, famine, and pestilence.<sup>9</sup> Even in the last war our army lost before Metz alone from abdominal typhus and dysentery, 2157 men; and who can reckon the instances in which, under the sickness endured there, the health of the most robust has broken down for the rest of the life? Only after years will the summaries of receding

facts be able to render adequate returns in this matter. But nowhere has the connection of the two fore-named diseases been so strikingly manifested, as in the North American succession war. Already in the first year, the number of sufferers from abdominal typhus reached 21,977, and the deaths 5608; in the second year the cases were 31,874, and the deaths 10,467. In the same space of time dysentery attacked 34,848 men, of whom 474 died.

In all such experiences attention is naturally directed to the ground as the source of miasma. The idea that a poisonous material, a malaria, springs out of the ground, and affects and kills men—so readily suggests itself, that it becomes reasserted with ever-increasing persistency, just like the suspicion of well-poisoning, in the history of diffused epidemics. Undoubtedly there lies at the base of the one, as of the other, a right instinctive feeling. But the latest enquiries have shewn for the first time, that in these sicknesses

there is least of all a question as to a poison which is essentially inherent in the soil, or which alien malice has imported into the wells. It is far more the men themselves who defile the ground, and thence outwards in all directions the water and the air, through their excretions, especially urine and ordure. All the stages of this corruption, this systematic infection of soil, water, and air, are not at present so accurately defined, that we could predicate every item of the process; but the main problem is solved, and growing acquaintance with the great questions of public health is unmistakably disclosed, in the ever-augmenting zeal of communities, for the cleansing of cities, in the urgent demand for sewerage, and removal of nuisances.

In war, as in peace, we equally observe that this kind of infection-tainting is not ordinarily the general property of all the soil or all the water; though the wind wafts us the miasma, nevertheless it pervades not the whole of the breeze; much

more there still remains on every infected ground, certain special areas, and most frequently are these human dwelling places. The simultaneous occurrence of noxious wound- and lying-in fever, in the hospitals and lazarettos, first drew attention to camp and house-miasma. By further observation, men became convinced that many a poison within the narrow walls of buildings or courts, springs from self-created impurities, which man is too lazy to remove at the proper time. Man ought consequently to charge his own neglect and ignorance, instead of seeking the cause of the mischief in alien culpability. Can it then be alleged that the dwelling-miasma, the hospital-miasma, the camp-miasma, are identical? That one and the same miasma produces according to circumstances abdominal typhus and dysentery, diphtheria and erysipelas, hospital gangrene and septicæmia? that it originates from the ordinary putrefactive organisms, and finds expression in particular fungi or algæ?

Billroth's assumption relative to the relationship of the above-named parasitic plants, as vegetation forms of *coccobacteria septica*, would lead with logical exactness to this conclusion, provided one generally attributes to these plants pathogenetic properties. But I might further add with equal right, ought cholera to be included? I have already evinced in my discourse of 1848, that the series of symptoms and of anatomical changes which one can excite in the blood of animals by systematic injection of putrifying fluids, resemble in the closest manner those which are evolved through the cholera poison. I inferred from this agreement, that the cholera poison must be nearly allied to the putrifying substances. It results therefrom, that the diphtheritic processes in cholera cannot be differentiated from those occurring in indigenous infection-diseases; that the so-called cholera fungis resemble even to the extent of being interchangeable with diarrhœa fungi.

It seems to me incontestible that, in the face of

these experiences, only two propositions are tenable. Either the micro-organisms of all the named infection diseases are identical, and then, as happens with Billroth, one can be relegated to special poisonous substances which are simultaneously present with the fungi and algæ, and take their origin independently of these; or the micro-organisms are different in spite of their apparent resemblance, and constitute the carriers and excitors in the body of the most perilous processes—they are the special causes of disease; a third conclusion seems to me quite inadmissible. The above mentioned two hypotheses are susceptible of a double application.

The special functions which are assigned to the micro-organisms can be diversely defined. It is supposable that these beings through their activity directly assail and destroy the living parts of the body, but also that they educe a noxious material, a poison which endangers life. Pacini thought the action of vibrios in cholera was according to

the former way. Similarly many recent enquirers as Huter and Klebs, suppose that in wound-fevers the monads or micro-sporons penetrate from the surface into a man's body, reach the white corpuscles of the blood or the blood itself, and by this medium are carried to other organs in order to exert in these their destroying activity; according to the second function, the parasitic body is not dangerous in a mechanical sense, but it is a poison-maker. Here again two distinctions are possible, according as the parasite grows a poison in itself and thus becomes poisonous; or eliminates the poison and yet remains itself innocuous. Otherwise expressed, this would signify, either there are among the microscopic fungi poisonous kinds such as have been long known among the large fungi, or the tainting organisms comport themselves towards the fouling substances, and the foul poison, as the zymotic fungi to the fermenting and the fermented substances; they are ferments.

One ought to appreciate clearly the great con-

trast between these possible inaugurating ways, in order to comprehend the danger of a one-sided statement. Neither of these modes of acting is merely imaginary; observation establishes for each mode a foundation of facts. The fungoid diseases of the outer skin, *e.g.*, “scald head” and of the superficial canals, as the thrush, and the fungoid affections of the external ear, are wholly of a local nature; here there is neither anything poisonous nor anything fermentative. Far more do these fungi act as disintegrating and demolishing, seeing they pervade and erode the human textures just as the dry rot attacks wood. One can suppose the working of fungi to be after an analogous manner, and an experiment of Grohe is very apt for shewing such a possibility to be admissible even for deeper corrosions. He inserted a germ of the largest yellow mould fungus, the *aspergillus*, into the tissues of a living animal, and a short time afterwards he noticed foci wholly built of aspergillus threads, in the most various



organs. These threads pervaded the texture, penetrated the parenchyma, and at last destroyed it. Quite similar according to Kleb's view is the working of the parasite, designated by him *microsporon septicum*, which he thinks he demonstrated to be the specific cause of noxious wound fever, in the lazarettos of the last war.

In this wise also carbuncle has been caused in recent times. In which malady the bacteria anthracis get into the blood and multiply to such an extent that, on the authority of one observer, a single drop contains 8 to 10 millions of bacteria, which by virtue of chemical affinity so far monopolise the supply of oxygen, that the blood corpuscles no longer breathe and the animal suffocates. Considered apart, a thoroughly plausible explanation; according to this theory, the blood-corpuscles become as it were besieged by the bacteria, which, cut off all subsistence from without. But experience teaches that carbuncle tainted blood is often very poor in bacteria. Even

in the last sickness among the roes of the Grunewald, I have inoculated rabbits with the yellow lymph which surrounded the neck lymphatic glands of the attacked deer, and which contained the tiniest and scantiest micro-organisms. Death resulted before the lapse of 24 hours from the insertion of these minute lymph foci, yet the blood of the dead rabbit exhibited hardly any parasites. The mechanical theory is therefore wholly untenable in this case. Moreover, when notwithstanding this default, the introduction of a single drop of blood taken out of the jugular vein of the affected rabbit, and planted in an incision made in the back of another rabbit, likewise sufficed to kill it before the expiration of the inoculating day; and when also the number of bacteria detectable in the blood was extremely limited; to my way of thinking the assumption of a chemical action remains as the only explanation.

I do not allege the necessary inadmissibility of

mechanical hypothesis; on the contrary, I hold them to be allowable and correct for certain cases. But I regard them as incorrect for the great infection diseases, at least for those of the epidemic kind. Far more accordant with my view is the assumption which I made in my discourse of the 2nd August, 1845, viz., that this is a question of chemical metamorphoses; an assumption which has found its expression in the establishment of ichorrhæmia and septicæmia, and which is alone to be sanctioned in reference to the great types of disease. That there are poisonous micro-organisms which, like the toad-stool, act noxiously when they are eaten, I will not controvert; specific facts, however, in support of this hypothesis do not seem obtainable hitherto in adequate number. There is consequently, as a *dernier resort*, the zymotic theory. Conformably to this the micro-organism through its vegetation, out of substances which it educes from the neighbourhood, and which it applies in the construction

and growth of its body, might evolve new products, whereby, a material with special injurious properties arises as waste and elimination matter. Thus the fungus of ergot of rye elaborates the so-called ergotin, a most energetic poison; thus the fermentation fungus elaborates alcohol, whose pernicious effects are sufficiently well known.

These secreted poisons are conceivably isolable from the micro-organisms which have hatched them, their activity is not tied to the presence of the fungus, any more than the fungus itself is of a poisonous nature. Yeast which consists wholly of ferment fungi, has been administered occasionally to the sick, in as large quantities as salad is eaten by the healthy, and yet no suspicious symptom followed. It is therefore well conceivable that on an inoculation spot, or on the site of a wound, a fungus focus forms in the human body, which secretes the large quantity of poison that not merely kills the neighbouring tissue, but also passes into the blood and lymph, and endangers

the life of the individual, without the fungus itself reaching the blood, and without the something that has reached blood, having in every instance a pathogenetic significance.

Since Panum has been enabled to isolate a newly-constituted poison out of tainted fluids, it can no longer be doubted that the corrupting infection, the septicemia, cannot be fairly referred to mechanical disturbance through micro-organisms.

It in nowise results that the production of foul-ing poisons is possible without the presence of foul organisms, or that we durst theorise on the former irrespectively of the presence of these. On the contrary, the more accurately we investigate, the more it appears that it is precisely these organisms which evolve the mischief. Certainly, it is necessary to beware of an unlimited application of this experience, such as even now obtains here and there, of carrying every ferment working to the account of fungi. Every organic cell has the

aptitude for drawing certain substances out of the neighbourhood, of absorbing, and elaborating, in order to its own development and function, whilst it often eliminates from itself effete and copious substances eminently noxious. By rich cell building, the magnitude of these metamorphoses attains vast extension, and there lies no absolute necessity to appeal to the aid of fungi for the production of waste material. Even where real ferment bodies are hatched, we find only normal cells at work. The history of the gastric juice specially inculcates this. The pepsin, as likewise the effective ferments of buccal and gastric fluid, are cell products, in whose elaboration, no fungus partakes. Whoever questions this may equally fairly conclude, that the effective parts of the spermatic fluid are not spermatic filaments but peculiar fungi. Thus pathology will ever maintain simultaneously with the efficiency of fungi, the efficiency of ordinary cells, and of the copious infecting material thereby produced as of pathogenetic moment.

This holds good not only for inflammatory processes, which recent eminent pathologists, as Burdon Sanderson, would likewise make amenable to fungus doctrine, in its widest range, but for syphilis, in which, in spite of most frequent research, and in spite of a probability grounded on the marked force of its contagiousness, up to the present moment, a microscopic element has not been actually established.

But such reservation is not permissible with the fouling processes. The micro-organisms here are everywhere demonstrable with facility, and their influence as movers of putrefaction is easy to be observed. The difficulty of understanding the pathological significance of this process, would then only appear insuperable, if in reality a single plant produced the manifold types of foulness-organisms.

But there remains in my mind, even in the very teeth of the seemingly surest verdict of morphological research, the decisive evidence based on

physiological and pathological results. Let the same form-elements evolve different workings, so must the elements be in reality different.

If we are unable to perceive directly this interior dissimilarity, in bodies so minute as the vibrios and bacteria, we ought to remember that, in the developing cells of eggs and numerous pathological growths, notwithstanding these appear as veritable giants by the side of vibrios, it cannot be predicated beforehand, what form of structure they will build. Indeed that the eggs themselves so closely resemble one another, the differences of the animals to be hatched out of them, cannot be divined even in the remotest degree. Therefore if it happen through inoculation, or through pathological accident, that by the instrumentality of bacteria closely similar to those of ordinary foul infusions, carbuncle arises, whilst the bacteria of ordinary infusions do not cause carbuncle, so must we inevitably conclude, that the bacteria of carbuncle must be as different from the bacteria of



infusion as hemlock from parsley. But in order to arrive at safe conclusions, science demands a certain multiplicity in the methods of enquiry. Only by a complete concordance of the various methods applicable in a given case, can the investigation be reckoned as decisive. If this concord remains unreachd, that method of proceeding, by which we can bring to bear the most effective aids and contrivances, will merit the preference. Acting according to this principle in forming a judgment about infection diseases, the morphological method will scarcely commend itself as the most suitable, since the application of the microscope comparatively soon reaches its limits, in the impossibility of a further optical solution of the mechanical structure of substances. Infection is determinable by experiment alone. To this we must bow. Moreover, the remaining disagreement between the morphological and the physiological method of enquiry may be only in appearance, for when the one method fails to give a satisfactory result,

owing to admitted inadequacy of the means used, one is not justified in ascribing to it so much consequence as to make a constant source of fresh doubts out of a manifestly unfinished investigation.

Man does not, however, readily concede that his means of obtaining knowledge are limited. Resignation is a difficult virtue. Nevertheless, it is indispensable to practice it, and eminently so in science.

It may seem paradoxical to demand renunciation from anyone who seeks full and complete truth; but the paradox consists only in the onesidedness of the way he follows: many ways lead to Rome but not all. And yet I, who in the beginning of my scientific career made the hard demand on scientific medicine, to base the demonstration of pathological processes on microscopic research, in order to approach these changes at least three hundred times nearer; now that one has approximated about 900 times or 1200 times nearer, I durst

well remember that, morphology represents only one side of biology, and that on the other side of morphology, lies a broad territory of mechanical and chemical processes, whose survey requires other means than morphology is able to afford.

Let it be further added that morphological rules do not and cannot give final conclusions as to the nature of things. As the ground of all human observation, even the conformation of religious conceptions, is dependent on the dominating ideas concerning the constitution of matter itself and the forces inherent therein; so is the last ground of pathological knowledge to be sought in physics and chemistry; these are the foundation sciences, and their importation into the daily use of physicians was the greatest and surest advance that medicine ever achieved. Therein lies the elucidation of the fact that in the short space since the founding of this Institution, a greater transformation has taken place in medical observation and treatment than in the 2,000

years from Hippocrates to Harvey. May the enlightened administration which has developed this beneficent foundation never forget that only by means of a liberal supply of every aid, can the development of the several students be so far advanced that, in the course of a few years of study, each may be able to learn the inestimable treasures of scientific experience, at all events in the main subjects ; that a sound acquaintance with at least the leading points, will alone insure the physician against the melancholy fate of being cut off from the knowledge of progressing science, and of being deprived of the possibility of helping himself. At least once in his life every scientific man ought to find himself thoroughly abreast of the present standpoint of his department and of the fundamental sciences ; otherwise he becomes superannuated before he has scarcely begun to act on his own responsibility. May our military directors while endeavouring to utilize every scientific gain in their department so circumspectly for the good

of the state, constantly have the conviction that, medicine as an applied branch of natural philosophy flourishes only on the broad ground of natural science; that on this ground she produces the richest fruits. Then shall we futhermore hope, that just as German medicine has been able to lead the van, German military medicine also, which already ranks so high, will continue to march onward for the welfare of the army, and the glory of the fatherland.





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